

CORRESPONDENCE.

To the Editor, JOURNAL OF THE EUGENICS EDUCATION SOCIETY.
Sir,

One important conclusion which I think must be drawn from Dr. Gibbons' paper in your issue of July, but which appears to have been overlooked by the Conference, is the astounding disgenic effect in itself of the curing of venereal diseases. For unless these are more common in the upper than in the lower classes (and I imagine the truth is very much the other way about) it is clear that Syphilis by rendering women sterile, and Gonorrhea by doing the same to men, act eugenically in reducing the descendants of the more foolish and less provident (fore-seeing) members of the community. This does not mean that we are to deprecate advance in medical discovery, but it does emphasise the urgent necessity of countering its disgenic effect in enabling weak and unhealthy people to propagate their kind by adopting every means possible to bring home to the ignorant, and the very poor and the feckless the ease and personal advantage to themselves of contraception, with the ideal for most of them of no children at all. The present lack of houses and of employment affords indeed a pertinent text.

At the same time an equally intensive campaign is clearly now required to induce those who can bring up a healthy family to do so. I was told, a little while ago, of one of our greatest public schools that among its old boys who fell in the war were 200 only-sons—that is to say in respect of this school alone 200 of our best families had hung their chance of continuance on the slender threads of one male life, which had now gone, and probably on the average less than one other. This from the national point of view is of course all wrong. The wise words of our President in his address on the "Future of our Race" need to be brought home to all educated and well-to-do parents. So much is heard now-a-days of the dangers of an increase in our numbers that it has come to be considered foolish for even a thoroughly healthy and able stock to have a family of more than three, and the great importance of breeding from our better strains is overlooked. There is probably no more important problem before England to-day than the substitution of its C. population by a mixture of A and B, but with a sufficiently intensive campaign the improvement in the course of one generation only, might be immense.

P. F. FYSON.

To the Editor,
Sir,

Mr. Perry, in your issue of July, after quoting instances to show that in the past advance in culture has usually been due to immigration of a more advanced people, and has disappeared with its dying out or departure, goes back completely in his last paragraph on the obvious conclusions, and on the teachings as I understand them of his "Children of the Sun" to argue that to-day with the wide diffusion of knowledge through printing, culture will survive independent of the people who made it. But a detail, like the change in the mode of communication of thought from the spoken to the written word, cannot upset a great principle if it be true; and Mr. Perry if he will look around may easily find to-day abundant evidence of the truth he was at such pains originally to establish in his "Children of the Sun." He need only look a few hundred miles to the West or to the East. The difference in culture attained by the Irish in the South of Ireland and the descendants of the immigrant Scotch in the North, or by the Danes in a country less kindly endowed by nature, are examples of a truth of which indeed none who have ever travelled and seen other civilisations can have the least doubt.

P. F. FYSON.

Sir,

Prof. Goodrich's review of my book is what I should expect from an old and valued friend and a scientific opponent. I have nothing whatever to complain of in its tone: my only reasons for commenting on it are first, that Prof. Goodrich has in one or two points misunderstood me and so, unintentionally no doubt, misrepresented me, and secondly, that the serious points on which we differ are in my

opinion vital and fundamental in the whole question of evolution. Prof. Goodrich has betrayed a want of acquaintance with recent work on the subject and so has been misled into giving a totally inaccurate account of the present position of opinion on these questions. Let me first refer to his misunderstandings. He takes me to task because on page 37 I talk of the transmission of male qualities to the offspring whereas he thinks that I should speak of "substances." But of course qualities inhere in substances and the importance of what is transmitted by the male lies not in the substance, which is infinitesimal in amount, but in the qualities or potentialities which go with it—it is these which constitute the marvel of heredity. Then again, Prof. Goodrich considers it "deplorable" that I should present such a travesty of the Darwinian position as to suggest that Darwin thought that natural selection alone could effect evolution. I fail to see how any careful reader of the book could extract such a meaning from what I said. If Prof. Goodrich had read page 112 he would have found these words: "Darwin . . . laid far greater stress on small differences of size, shape and colour . . . these are known as fluctuating variations and Darwin assumed that they were inheritable, and in this assumption he was followed by Alfred Russel Wallace who shares with Darwin the glory of having first clearly enunciated the principle of natural selection." Darwin postulated these variations in all directions and on that basis assumed the all-importance of natural selection. With all respect to Prof. Goodrich, I think that it is a rather difficult thing to "catch me out" in the *Origin of Species* for I made that work the special text-book for my advanced students for nine years and went through it with them every spring from cover to cover.

I now come to the serious differences between Prof. Goodrich and myself. He voices three complaints against me: first, that I have not stated that the current "factorial hypothesis" accounts for a vast number of facts for which no better explanation has been advanced; second, that I lay stress on the result of the experiments of Kammerer, Durkhen and Pavlov, which are not generally accepted and have not been confirmed; and thirdly, that I have made no mention of the "overwhelming case" against Lamarckism.

As to the factorial hypothesis as a development of Mendelism the position which I took up in the book is that it explains everything in heredity; which is the same as saying that it explains nothing. When the so-called explanation consists in inventing innumerable imaginary "factors" to fit every result, and when fresh factors are invented to account for every deviation from the expected result, then it becomes evident that we are dealing with mere verbiage, and the whole thing becomes, as Goldschmidt in his book on "Sex" has wisely remarked, purely formal. Johanssen, the discoverer of the "pure line," has recently expressed an ardent wish that the term "unit-character" (=factor) should be abolished. "What do we mean by it" he asks, "but a disturbance of the chromosomes?" which is exactly the position which I adopt. There will be no progress in understanding mutations until some advance in discovering their underlying physiological basis has been achieved, and it is because Tornier's work seems to throw light on this basis that I have emphasised it in the book. The object of the book is not merely to give results about which everyone is agreed, but to show where light is breaking on the subject, to stimulate thought and further research.

I turn now to the subject of Durkhen's and Kammerer's experiments which demonstrate the inheritability of acquired character which Prof. Goodrich says are not accepted because "over and over again similar claims have been made by authors whose observations in the end have turned out to be erroneous or capable of a better explanation."

This statement I must meet with a point-blank denial. I challenge Prof. Goodrich to produce any previous experiments like those of Kammerer and Durkhen, carefully thought out, and patiently carried through in order to put to a crucial test the inheritability of acquired characters. Sporadic observations in favour of Lamarckism have been from time to time adduced and these the Weismannian school have endeavoured to explain away; but it is characteristic of the decisive character of Kammerer's work that no one has endeavoured to "explain it away." The criticism of those who refuse to accept it has been that the observations have been deliberately falsified. But Durkhen's work done on a totally different animal at a different university leads to precisely the same results as those at which Kammerer arrived and affords as complete a confirmation of Kammerer's work as could be desired. Kammerer's work occupied 11 years and Durkhen's 5 years,

and it is idle for critics like Prof. Goodrich to sit back in their chairs and demand confirmation of results which are distasteful to them when they themselves will do nothing to test them. For the last five years I have been endeavouring to repeat Kammerer's work on the Salamander, and though through inexperience I have failed to get the animals to breed, I have confirmed his results as far as the first generation is concerned, and *it is only with respect to this generation that Kammerer's work has been criticised by other observers.*

Lastly, Prof. Goodrich complains that I have failed to present the "overwhelming case" against Lamarckism. I have analysed in detail this "overwhelming case" in an article which was published in "Science Progress" in 1921. I have shown that the case rests chiefly on the *a priori* statements and dogmatism of Weismann and that the props of the whole "germ-plasm" theory have been destroyed by subsequent research. To this article I may perhaps direct Prof. Goodrich's attention. Weismannian doctrine so imposed on zoological thought in the "nineties" that it became a tradition of zoological teaching: both Prof. Goodrich and I (for we are contemporaries) were brought up on it, but whereas I have emancipated myself from it, he has apparently not yet succeeded in doing so.

Now finally as to the general opinion on Lamarckism as a theory of evolution. The theory of evolution so far as animals are concerned is based on three classes of facts, viz: palæontology, embryology and systematic zoology, which last may be termed detailed comparative anatomy. So far as systematic zoology is concerned, my friend Dr. Tate Regan, Keeper of Zoology in the Natural History Museum, and an unrivalled authority on the systematic classification of fish, refuses it is true to call himself a Lamarckian, but he takes up a position which I for one, find it impossible to distinguish from Lamarckian. For he asserts that the decisive factors in evolution have been "habitual changes," that is the assumption by different members of the same stock of different habits—and that changes in habits have preceded and caused changes of structure. As to embryology, about which I can claim special knowledge, I need only say that I began my work on the Comparative Embryology of the Invertebrates as a non-Lamarckian and that I was driven to Lamarckism by a survey of the facts which I had to review before I had heard of Kammerer at all. Turning now to palæontology, I may say that I was recently shown a letter from a leading palæontologist of N. America in which the author stated "All we palæontologists are Lamarckians," and I find that palæontological opinion in Cambridge adopts precisely the same attitude. Nay more, Dr. Bateson himself in a recent discussion in which he and I participated, openly admitted that he had given up the belief that mutations were factors in evolution and that he attached most importance to the orthogenetic "trends" which palæontologists had brought to light. But if we refer to Eimer who put forward the theory of orthogenesis, we find that he attributed these trends to the persistent action of the environment on the constitution of animals, and that is in effect Lamarckism. I have tried to show that those most fully conversant with the facts to be explained are all tending towards Lamarckism, that we have now detailed and decisive experimental evidence in favour of it, and that if things go on as they are doing Oxford may become in Zoology, as it is in other subjects, the home "of lost causes and impossible loyalties."

E. W. MACBRIDE.